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Published in:
Developmental Science

DOI:
10.1111/desc.12738

Publication date:
2019

Document Version
Peer reviewed version

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We (Meltzoff et al., 2018) described how Oostenbroek et al.'s (2016) design likely dampened infant imitation. In their commentary, Oostenbroek et al. (in press) argue that our points are post hoc. It is important for readers to know that they are not. Our paper re-stated "best practices" described in published papers. Based on the literature, the design used by Oostenbroek et al. (2016) would be *predicted* to dampen infant imitation.

First, Oostenbroek et al.'s (2016) test periods were too brief. The stimulus presentation for each type of gesture was too short to ensure that neonates saw the display. The response measurement period did not allow neonates sufficient time to organize a motor response. Meltzoff and Moore [M&M] (1983a, 1994) introduced experimental procedures *specifically designed* to address these issues (also, Simpson et al., 2014). Oostenbroek et al. did not capitalize on these procedural advances.

Second, Oostenbroek et al. allowed uncontrolled experimenter-infant interactions during the test session itself. Previous papers on imitation provided analyses of how uncontrolled interactions with the experimenter can introduce "noise" in experiments of facial imitation (M&M, 1983b, 1994).

Third, Oostenbroek et al. used suboptimal eliciting conditions. Neonates cannot support their own heads; in Oostenbroek et al., infants' heads were allowed to flop from side-to-side unsupported on the experimenter's lap while the experimenter gestured with both hands. In addition, papers have listed techniques for maximizing visual attention (controlled lighting, homogeneous background) (M&M, 1994). Oostenbroek et al. tested infants on a couch in the home.

Despite a design that would blunt imitation, our re-analysis of Oostenbroek et al.'s data showed a response pattern that is consistent with the imitation of tongue protrusion (TP). In their commentary, Oostenbroek et al. (in press) now propose limiting analyses to a subset of their original controls. We re-analyzed their data accordingly. Again, the results support early imitation. The cross-sectional data (their Table S4) collapsed across age show significantly more infant tongue protrusion in response to the TP demonstration than to the mean of the six dynamic face controls (mouth, happy, sad, mmm, ee, and click): $t(104)=4.62$, $p=.00001$. The results are also significant using a narrower subset of stimuli (mouth, happy, and sad): $t(104)=3.20$, $p=.0018$. These results rule out arousal, because the adult TP demonstration was significantly more effective in eliciting infant tongue protrusions than the category of dynamic face controls. Tongue protrusion matching is a robust phenomenon successfully elicited in more than two dozen studies (reviews: M&M, 1997; Nagy et al., 2013; Simpson et al., 2014).

There are more general lessons to be drawn. Psychology is experiencing what some call a "replication crisis." Those who attempt to reproduce effects have scientific responsibilities, as do original authors. Both can help psychology become a more cumulative science. It is crucial for investigators to label whether or not a study is a direct replication attempt. If it is not a direct replication, procedural alterations and associated limitations should be discussed. It sows confusion to use procedures that are already *predicted* to dampen effects, without alerting readers. Psychology will be advanced by more stringent standards for reporting and evaluating studies aimed at reproducing published effects.

Infant imitation is a fundamental skill prior to language and contributes to the development of social cognition. On this both Oostenbroek et al. and we agree.

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